ne ne pedese sivona (ascrahifetse proceso). Vilsono ne reque evit e al se i La 1986, atama . La saferra de la mezida en el proceso se N**evamber, 23, 1951**

It's Research Lab., USPHS that is a linear reference of the service of the servic of Man Yorks 21, N.Y. Such assessment of the same of t The west of the consider a results state year on a single result of section of

ord out of Dear Bergie: the speciments of the state of th Thank you for the sample of washed agar (rec'd airmail!) and your is flatter of the Alst with its interesting information of Qunny and Roger wanted to Look for Pseudospnes mutants requiring the Pyravate Oxidation Pactor, and I had sent them PF-11 as a previous unknown, so that your information (which I shall relay promptly) will be quite useful. and the world must be the state of our case of the it to all the control of the state of

్రంకేంది. అదేవల పేపలుందిను కాట్లు ముక్కుడు. ప్రాకెట్టి కేస్కి ప్రాకెట్టుకున్నారు. మహ్యుకు గణానాహ్యాన్నారు. కేస్టంఖోతి ముద్దులో ఉమ్మూర్కాన్నులో కామాలు అద్ది కామాలు కేస్తున్నారు. మందు కాట్లు కేసాన్ తెస్ పేస్తు

As to RF-21, I should be willing to accept your judgment on writing a note, and as to whether you should appear as sole or mentor author. If you do think itbworthwhile (and the industrial application of the coli mutant points to the utility) to advertise it, I would suggest sending the note to Archives, and unless you want to assume the full burden of its distribution, also depositing the mutant with the ATCO (if they can be relied on for such things). The leucine requirement is revertible, and it might be worthwhile to get it out, both for simplification of the medium, and possibly to help if the leucine-requirement has something to do with the inhibitions. The mutant is derived from Stanier's A3.12 strain (J. Bact, 59:117-127 1950, 54:339-248 1947, and especially 55:477-94 1948). Following enrichment by the pehicillin method, the mutant was detected by means of replica plating.

My prompt response is directed mainly at your editorial. I think the biological approach to bacteria should be hammered home at every possible occasion. However, my own experience is that it takes a year to reach a proper philosophical understanding of Luria and Delbruck's analysis. This was the main reason (Harry Eagle was the second) for the note "Replica plating and indirect selection of bacterial mutants" by J. and E.M. Lederberg, which is due of the January 1952 issue of J. Bact. The success of indirect selection obviates the difficulty you pose on p. 4, Kine 6-10 old would not go so far as to suggest you medify your editorial at this late date; but I would not be unhappy to have indirect selection interpreted to your Masses at this or any other occasion. a (By the way, I think your leg is being pulled over the footnote p.5, unless you mean "interpretation of the experimental results". See p. 493-494 of their paper.) I enclose an essentially final draft of the ms. Back to the editorial, why don't you use this occasion to clear up Graessle & Pietrewski's paper on the interaction of sm a-(J. Bact. 57:459)?

Whilewwe're discussing antibioties, I'd like to tell you about something that's either infuriating or amusing, according to how you look at it. You know the paper by Smith, Oginsky, and Umbreit, in the series on sm action mechanism, dealing with metabolism of Sr strains (J. Bact. 587661). Roger was particularly impressed by the statement that Sr strains are not improved in growth by aeration. This was not in accord with my own experience, so after verifying that our Sr were not distinguishable by this test arom S, I wrote the Merck people asking for their cultures for verification. Some time later, I received their "Murray" strains, stander, with a note stating that the published metabolic differences had been checkedbut not the agration effect: they were busy with other problems. In my hands, the s and r strains were no different, and I wrote back accordingly. I was astonished to receive a reply that they were not surprised at this, for they had run into the same thing many times since their original experiments. (In my Tiret letter I had specifically asset for strains to verify the acration effect). They claim, however, that after continued oultivation four six mos. some of the atrains did give rise to the non-herobic type - would I be interested to try that! They admit that they "do not know whether lit is a matter of chance that the organism loses the ability to respond to air". If I could have found all or this out in the first place, it would not be soldisturbing. Not to change the subject very drastically, I was pleased to see llough's note in PNAS, but if you is think he common the earthouseros need even bigod en heim ob a st **unita**no ira cue la esta Todi cesti.

Stander had argood idea about this (because he bedieved what he read), " which unfortunately won't work: that the non-aerobic type discrimilar to the petite yeart of Ephrussi's, and results from the direct action of sm on the oridation system. The sponutineous Sr mutants are able to survive selection with 8 but me stripped of the system. In K-12 this could have been tested genetically; otherwise, 5 obtained by indirect selection would have given a very appropriate test. too bad. Anyhow, it might still be a good idea to look out for analogous forms. The est to the all the lang and he described

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Japanings is specimental limit. In the session by I go be added India colf or day P.S. Methiomanistraineless has occurred at least six times as a case-step auxotroph. Heven't you ever hit ith Perhaps it occurs less often in W. It has also and come up in Salmonella. I be see the the line of the set of the set of the set စေသို့အသည့် မြို့ကြသည်။ ကြည်သည့် သို့အလေးသည်။ မွဲ့တွင် ချိန်းမေးလည်းခဲ့သေသည်မေးသည် သို့သည်သော သူမေးမေးသည် အသည် မြိန်သည်မေးသည်သည် သည် သည် သည် သည် သည်မေးသည်သည်။ သည်မေးမေးအရှေ့ စေး**သည်သည် ခုန့်နှင့် အသ**ည်သည်။ သို့ မြန်လို့ စမှာတ

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